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In-kind Conservation Payments Crowd in Environmental Values and Increase Support for Government Intervention: A Randomized Trial in Bolivia

ABSTRACT

There is growing use of economic incentives such as Payments for Ecosystem Services (PES) to encourage sustainable land management. An important critique is that such approaches may unintentionally disrupt environmental and social values, 'crowding out' pre-existing motivations to conserve. Some scholars suggest that the use of in-kind payments and norm-based framing, rather than financial transfers and a market framing, can mitigate these risks. There are calls to use more robust methods for impact evaluation in environmental policy. We use one of the only Randomized Controlled Trials of a conservation incentive scheme to evaluate its impact on self-stated environmental and social values and beliefs. Data from before and after the intervention, from households in villages randomly selected to receive the program or not, demonstrate that the program increased prioritization of environmental values (evidence of crowding-in as opposed to crowding out) and altered social beliefs related to inequality and the role of government. The findings demonstrate that this conservation program had a positive impact on environmental values and increased the belief that government involvement is appropriate. The scheme, with its use of in-kind payments and reciprocity framing, offers lessons to those seeking to develop effective schemes to incentivize positive environmental stewardship.

Keywords: payments for ecosystem services; motivation crowding; social norms; framing; environmental values; Bolivia

1. Introduction

Payments for Ecosystem Services (PES) are voluntary transactions whereby land managers are incentivized to carry out natural resource management actions believed to generate ecosystem services for another group of users or society as a whole. PES schemes have proven to be a valuable tool for promoting conservation in vulnerable and critically important ecosystems (Jayachandran et al. 2017). This approach has been adopted worldwide (Kinzig et al. 2011; Pattanayak, Wunder & Ferraro 2010) with over 550 ongoing PES programs, representing around US$36-42 billion in transactions (Salzman et al. 2018).

A common critique of PES programs is that financial incentives may have adverse effects on pre-existing motivations for conservation, including both environmental and social values and beliefs (Rode, Gómez-Baggethun & Krause 2015). In particular, critics view PES as engaging in "commodity fetishism", reducing complex ecosystem functions to tradeable services (Kosoy & Corbera 2010, Muradian et al. 2010). A large body of social science research demonstrates that financial incentives sometimes result in the opposite of their intended effect (Bowles 2008; Deci, Koestner & Ryan 1999; Frey 1994; Gneezy & Rustichini 2000; Titmuss 1971). This phenomenon, known as "motivation crowding", could potentially lead to a *decrease* in conservation behavior, especially after the incentive payments end (Andersson et al. 2018). However, the original psychology literature on motivation crowding suggests that external interventions can sometimes enhance pre-existing motivations rather than displace them (crowding "in" rather than "out"), if the external intervention is perceived as congruent with one's own values and identity (Deci, Koestner & Ryan 1999).

Another, closely related critique of PES programs is that, in practice, they often present barriers to entry that exclude the poorest members of communities (Bremer, Farley & Lopez-Carr 2014; Pagiola, Arcenas & Platais 2005). Compensation programs often feature participation skewed toward wealthier members of a community (Greig-Gran, Porras & Wunder 2005; Zbinden & Lee 2005), and there is a risk that PES schemes may exacerbate pre-existing inequalities between landowners and others (García-Amado et al. 2011). The tension between equity and efficiency of market-based mechanisms has been an important concern in the literature (Brown & Adger 2007; Landell-Mills 2002; Pascual et al. 2010; Wunder 2008). Some caution that a failure to consider social equity can undermine environmental protection in the long-run through disenchantment with the program (Pascual et al. 2014). These studies raise concerns about unintended social impacts that may arise from PES programs, leading scholars to a call for greater inclusion of the poor in PES (Farley & Costanza 2010). They also raise questions about the potential for motivation crowding with respect, not only to intrinsic environmental values, but also to pro-social values and beliefs, especially as they relate to inequality.

Apart from direct material benefits provided to participants, policies and programmatic interventions also have interpretive effects (Pierson 1993) which may shape participants' "psychological predisposition to participate in public life" and perceptions of "their status in relation to other citizens and government" (Mettler 2002, p.352). Studies of this dynamic interaction, aka policy feedback, have largely been focused on social policy in the United States and Europe (Béland 2010, Campbell 2012), with little attention to the developing country context, where government policies often co-exist and overlap with programs implemented by international

and non-governmental organizations. If PES influences the values and beliefs of community members, this may have long-run implications not only for the specific conservation behavior it was designed to influence, but also for subsequent policy efforts related to equity in the same communities. This represents a potential spillover of motivation crowding to other areas of policy intervention, and warrants attention to the influence of PES on beliefs and values related to inequality and government intervention, in addition to environmental conservation.

The introduction to a recent special section on motivation crowding in *Ecological Economics* (Ezzine-de-Blas et al. 2019) argued that particular programmatic design features of PES programs, including for example payment type and communication, will influence the likelihood of crowding in vs. crowding out, to the extent that they stimulate feelings of competence, autonomy, and social/environmental relatedness. Prior laboratory research suggests that in-kind payments may be less prone to crowding out than cash, likely because they evoke social norms rather than a "market logic" (Heyman & Ariely 2004). Scholars have suggested such in-kind payments may be more effective in the application of PES (Kerr, Vardhan & Jindal 2014, Chan et al. 2017) but this has not been tested in a field-based experiment.

In addition, a growing literature suggests that simply framing an intervention in a particular way can change how people react to it (Chong and Druckman 2007, Clot et al. 2017) and that the effectiveness of a particular framing depends on pre-existing norms and beliefs (Andrews et al. 2013). One such pre-existing norm is reciprocity, or the relational notion that people should give back to those who help them. Recent research exploring the drivers of environmental values has shown that they can be driven by a perceived relationship with nature (Bremer et al. 2018, Chan

et al. 2016, 2017). Reciprocity is considered one of several shared principles of moral psychology, common across many cultures (Haidt 2007) and has been observed to motivate human behavior in a variety of decision contexts (Axelrod & Hamilton 1981; Falk & Fischbacher 2006). Taken as a whole, this suggests that the combination of in-kind compensation and reciprocity framing may reduce the risks of motivation crowding in incentive schemes.

A blossoming literature has explored the psychological impact of PES, and PES-like, programs on individuals, through their motivations, values, beliefs and internalized norms, with few consistent results. This literature has included a range of methods including ethnographic analyses (Bose, Garcia & Vira 2019, Van Hecken et al. 2019), structured interviews (García-Amado, Pérez & García 2013), quasi-experimental approaches (Agrawal, Chhatre & Gerber 2015; Chervier, Le Velly & Ezzine-de-Blas 2019), regression discontinuity designs (Alix-Garcia et al. 2018) and framed field experiments (Andersson et al. 2018; Cook et al. 2019; Handberg & Angelsen 2019; Kaczan, Swallow & Adamowicz 2019; Moros, Valez & Corbera 2019, Kolinjivadi et al. 2019). There is substantial interest in the use of Randomized Controlled Trials (RCTs) in conservation (Ferraro & Pattanayak 2006, Bayliss et al. 2015), where units are randomly allocated to receive an intervention or not, as a robust method of impact evaluation (Banerjee & Duflo 2009). RCTs overcome many of the challenges of other approaches to allow causal inference (the ability to conclude that the intervention resulted in the result observed). However, the use of randomized trials is still very rare in the study of environmental management interventions (Ma et al. 2017). There are only two published RCT evaluations of PES schemes (Jayachandran et al. 2017; Pynegar et al. 2018) and none that examine psychological effects on the values and beliefs of participants.

156 We present the results of the only Randomized Controlled Trial to date that measures the effects
157 of a conservation incentive scheme on environmental and social values and beliefs among
158 community members. In this paper, we experimentally evaluate the impact of a PES-like program,
159 called *Watershared*, that features two specific design features intended to reduce the risk of
160 motivation crowding: the use of in-kind payments and framing that references local reciprocity
161 norms. We use before and after data from households in communities randomly allocated to be
162 offered *Watershared* agreements (treatment communities) or not (control communities) to evaluate
163 the extent to which the scheme resulted in motivation crowding related to environmental and social
164 values and beliefs.
165
166 **2. Environmental and Social Values and Beliefs**
167 The theory of motivation crowding primarily focuses on how motivation for future behavior will
168 be affected after incentive programs end and the new, external motivation is no longer a direct
169 driver (Andersson et al. 2018). However, as they have not yet happened, future environmental
170 behaviors and the motivations behind them are difficult to observe directly. As a result, studies of
171 motivation crowding often focus instead on values and beliefs which are commonly understood to
172 be important precursors to motivations for pro-environmental behaviors. If PES and PES-like
173 programs affect motivations for future environmental behaviors, they likely do so through changes
174 to individual values and beliefs.
175
176 Terms such as values and beliefs can be used to mean subtly different things. Our goal is not to
177 contribute to the theoretical arguments relating to these definitions but to examine shifts in mental
178 assessments that people might make as the result of experiencing a PES or PES-like intervention,

179 and which, in turn, could influence later motivations for environmental behavior. Values can be
180 understood as universally held guiding principles for decisions that people make in their lives
181 (Schwartz 1992, p.21), thus providing a direct precursor to motivations for behavior. Much of the
182 literature on motivations for environmental behavior, specifically, focuses on four key types of
183 values: (i) hedonic or short-term pleasure-seeking values, (ii) egoistic or market values, (iii)
184 altruistic or pro-social values and (iv) biospheric or environmental values (Steg & DeGroot 2012,
185 Steg et al. 2014b). Given the importance of both environmental and social values in motivating
186 environmental behaviors (Rode, Gómez-Baggethun & Krause 2015), there is a risk if either or
187 both are crowded out by financial incentives.
188
189 There is evidence that those who endorse either environmental or social values are typically more
190 motivated to engage in pro-environmental behaviors (Steg et al. 2014a, Steg et al. 2014b, Nordlund
191 & Garvill 2002, Stern et al. 1995, Thogersen & Olander 2002). However, it is well recognized that
192 values alone are insufficient to motivate action. In order to take action, people must not only place
193 value on something, but also hold related beliefs, for example believing that the thing they value
194 is affected through their own individual actions (Schwartz 1970, 1977, Stern et al. 1995). We
195 define a belief as "any proposition that is accepted as true" (Colman 2001, as cited in Kenter et al.
196 2015), which is broad enough to include both value-laden attitudes and norms, as well as mere
197 descriptive perceptions of the world. The particular beliefs we measure in this paper are those that
198 seem most directly related to PES interventions: perceptions of a trade-off between environmental
199 conservation and economic growth, and views on inequality and egalitarian norms (see Table 1).
200 These touch directly on the two primary critiques of mainstream PES: (i) that they may lead to
201 "commodity fetishism" whereby perceptions shift toward viewing the forest as an economic

commodity, and (ii) that they may exacerbate inequalities in communities, with related impacts on perceptions of those inequalities. The intervention we examine here made explicit efforts to avoid these pitfalls through its use of in-kind incentives and reciprocity framing.

3. The Intervention: *Watershared*

In 2003 the non-governmental organization Fundación Natura Bolivia (Natura), in cooperation with several municipal governments, began using in-kind incentives to encourage conservation in the Andean region of Bolivia. Their program, now called *Watershared,* aims to slow deforestation and maintain supplies of high quality water available to communities. The program provides modest development support in exchange for avoiding deforestation and excluding livestock from riparian forest. Natura first visited each treatment community to offer a series of information sessions presenting their compensations as "reciprocal watershed agreements" and likening the arrangements to existing reciprocity norms that are common in the region (Bétrisey & Mager 2014; Capuma 2007). The information sessions characterized the program as establishing a reciprocal relationship between (i) Natura and those entering into the *Watershared* agreements, (ii) upstream and downstream water users, as well as between (iii) human beings and the natural environment. The original definition of PES involves buyers and sellers of services (Wunder 2007), while *Watershared* simply incentivizes landowners to conserve their watersheds. However, the intervention does involve "voluntary transactions between service users and service providers that are conditional on agreed rules of natural resource management for generating offsite services" (Wunder 2015) and so consideration of the *Watershared* scheme is relevant to those interested in the design of conservation incentive schemes such as PES. As of 2016, 210,000 hectares of forest

owned by 4,500 households were under some version of Natura's *Watershared* conservation agreements (Asquith 2016).

In our study setting, households enrolling land in Watershared agreements were provided with development projects with a value of $100 (as a one-off enrollment bonus) plus a variable amount (ranging from $1-$10) per hectare conserved, depending on the type of land and the rules they agreed to follow, which could include restrictions on both deforestation and degradation due to cattle grazing. Between the 1960s and early 2000s, deforestation in the Bolivian lowlands increased from about $4.7x10^4$ hectares/year to more than $2.9x10^5$ hectares/year (Killeen et al. 2008). During the ten years prior to our baseline survey, deforestation in our specific study area was approximately 4,147hectares, with a mean deforestation rate of 1.2% per community (Wiik et al. 2019). The goal of the *Watershared* program was to limit forest degradation, as well as deforestation. In particular, the agreements targeted the issue of cattle grazing in the watershed, which can lead to fecal contamination of the water source (Crane et al. 1983, Sunohara et al. 2012) and creates risks for biodiversity (Stern et al. 2002).

Payments were made in the form of inputs for sustainable livelihoods, such as fruit trees, beekeeping equipment, irrigation tubing, or barbed wire (to help enclose the cattle and keep them away from the watershed). Agreements (for three years) were offered on an individual basis (Pynegar et al. 2018). Previous research on *Watershared* found that take-up was determined by a combination of financial and social characteristics, with poorer community members less able to participate (Grillos 2017) and that those motivated by pro-nature instrumental motivations were more likely to enroll land which resulted in additional conservation (Bottazzi et al. 2018). In this

247 study, we examine whether experiencing the intervention has changed prioritization of
248 environmental and social values, and whether it affected self-stated agreement with normative
249 statements related to inequality and the environment.
250
251 **4. Research Design**
252 **4.1 A Randomized Controlled Trial in Bolivia**
253 The *Watershared* intervention we study here took the form of a randomized controlled trial within
254 the Río Grande Valles Cruceños (RG-VC) Natural Integrated Management Area. The RG-VC is a
255 mixed-use protected area, meaning that, while it is identified as an important ecosystem in need of
256 protection, the government also recognizes the rights of pre-existing forest dwellers to use their
257 own land as they deem appropriate. Natura identified 129 villages inside the RG-VC and
258 conducted a pre-intervention survey with households in all of those communities in late 2010.
259 After stratifying by municipality, village size and number of cattle in the community, they then
260 randomly selected 65 villages out of the original 129 included in the survey.[4] Individuals in these
261 randomly selected villages were offered the opportunity to enroll their land in *Watershared*
262 agreements, while the remaining communities constituted a control group (Pynegar et al. 2018).
263 Five years later, in late 2015, we implemented a follow-up survey with the same households in all
264 villages (those that received the program as well as those that did not), generating a panel dataset
265 (Bottazzi et al. 2017). Two papers have been published using the RCT. Pynegar et al. (2018)
266 examined the impact of the intervention on water quality (in terms of *E. coli* contamination of
267 water used for human consumption) and found no impact. Wiik et al. (2019) showed that the

[4] The study sample originally involved 130 villages, but one of the randomly selected control villages later turned out to be located outside the designated study area, so the baseline survey was not conducted there and it was dropped from all analyses.

intervention had limited impact on slowing deforestation (using the Global Forest Change data). Ours is the first paper to make use of the household survey data related to this intervention.

The randomized design of the intervention eliminates concerns over selection bias (Duflo et al. 2007), and balance tests confirm that the treatment and control groups did not differ substantially at the outset on neither demographic characteristics nor our key outcome variables (See Appendix A). However, those sampled in the treatment group were less likely to be active members of the community council (called the *organización territorial de base*, or "OTB"), which previous research also cited as an important predictor of program take-up (Grillos 2017). We address this issue in the analytic methods section.

4.2 The Dataset

Two thousand, six hundred and one (2,601) households were included in the pre-treatment baseline survey. Of these, 55% (1,443 households) reside in one of the 65 treatment villages, and the other 45% (1,158) reside in one of the 64 control villages. Of those initially surveyed within treatment villages, 38% (548 households) took up *Watershared* agreements. Since some families live in one community but simultaneously own land in another, there was a small amount of contamination in the control group, with 32 (out of 1,158 control households) reporting they took up a *Watershared* agreement. We directly address this two-sided noncompliance in the analytic methods section below. The post-treatment endline resurveyed 1,672 of those covered in the baseline. Attrition was due to a combination of people moving away (there is high rural depopulation in this part of

289 Bolivia) and not being available.[5] Attrition was not correlated with any of our key outcome
290 variables, although the subset that was re-surveyed at endline did differ on some control variables.[6]
291 Of those households surveyed at both baseline and endline, 58% (970 households) were in the
292 treatment group, and 38% (548 households) of those had entered into *Watershared* agreements.
293
294 The full survey instrument is archived alongside the full dataset [dataset] (Bottazzi et al. 2017).
295 The full text of the particular questions we identified as measuring environmental and social values
296 and beliefs is included in the next section (translated into English). Some of these questions were
297 initially removed from the post-treatment survey because of concerns about the length of the
298 survey. Due to their scholarly interest, they were then reintroduced in the remaining surveys. For
299 this reason, the sample size for some of these analyses is much more limited than the full set of
300 households included in the more general survey. There were 333 households that received the full
301 set of all our values and beliefs questions at both baseline and endline (i.e. 666 observations in the
302 panel dataset), and 69% of these (231 households) were part of the randomly assigned treatment
303 group. Of those in the treatment group, 40% (92 individuals) had taken up *Watershared*
304 agreements. Balance tests confirm that this smaller subsample is representative of the broader
305 study region based on statistics from the full baseline survey (See Appendix B).[7]
306
307 **4.3 Outcome Measures: Values and beliefs**

[5] In addition, some additional households were also picked up in the endline survey without having been included in the original baseline survey, but these do not figure into any of our analyses or tables and represent less than 3% of the total households with whom we made contact throughout this process.
[6] Attrition was associated with, on average, less cattle ownership, slightly fewer people in the household, and less OTB membership. See Appendix B for comparisons across subsets.
[7] The two groups did differ slightly in that the smaller sample (who received all the questions at endline) oversampled the treatment group relative to the true proportions (69% of the households included in the values sub-sample resides in the treatment group villages).

The survey included questions about demographic characteristics, assets, education and livelihoods as well as questions relating to environmental and social values and beliefs. Table 1 includes the full text of the survey questions (translated from Spanish) used to construct our outcome measures related to environmental and social values and beliefs.

Table 1: Survey Questions on Environmental and Social Values and Beliefs
(*Original Spanish in Italics*)

Construct	Survey Question
Values	I'm going to present you with some values that may be taught to children in the home. Of these values, can you choose the two that you think are the most important? (a) Independence, (b) Creativity, **(c) Protecting the Environment, (d) Sharing with Others, being altruistic** (e) Obedience, (f) Being a Good Student, (g) Being Successful *Voy a presentarle algunos valores que se puede enseñar a los niños en casa. ¿De estos valores, puede elegir los dos que piensa que son los más importantes?* *(a) Independencia, (b) Creatividad,* ***(c) Cuidar el medio ambiente, (d) Compartir con los demás, ser altruisto,*** *(e) Obediencia, (f) Ser un buen estudiante, (g) Búsqueda del éxito*
Beliefs	Now I will read some statements and I would like to know if you agree with each one. There is no correct answer, I just want to know your opinion. [1= completely disagree… 5= completely agree] *Ahora voy a leer unas afirmaciones y me gustaría saber si usted está de acuerdo con cada una. No hay una respuesta correca, sólo quiero saber su opinión sobre cada una de las afirmaciones.* *[1=completamente en desacuerdo… 5=completamente de acuerdo]*
Environmental beliefs	• "In order to improve quality of life, it is necessary to harm the environment." [*"Para mejorar las condiciones de vida, es necesario dañar el medio ambiente."*] • "We can have higher economic incomes if we protect the environment." [*"Podemos tener mejores ingresos económicos si protegemos el medio ambiente"*]
Social beliefs	• "It is the responsibility of the government to reduce income inequality between people with a lot of money and people with little money." [*"Es responsibilidad del gobierno reducer la desigualdad de ingresos entre las personas con mucho dinero y las personas con poco dinero."*]

	• "If a person works more than others, it's fair that they earn more money." [*"Si una persona trabaja más que otras personas, es justo que gane más dinero."*] • "If a person earns more than others, they must share with the rest." [*"Si una persona gane más que otras, tiene que compartir con los demás."*]

To measure the relative priority placed on environmental and social values, we included a question in the survey that asked respondents to choose their top two priorities from a list of values that could be taught to children in the home. Among this list of possible values were the options "protecting the environment" (biospheric/environmental values) and "sharing with others" (altruistic/pro-social values). Our outcome variables related to values were two binary variables: whether an individual chose, respectively, protecting the environment (environmental values) or sharing with others (social values), as one of their top two priorities. This question was adapted for the local context from one that appeared on the World Values Survey questionnaire (Inglehart et al. 2014).

While environmental values have been measured in variety of ways in the past (Dietz 2005), we find this relative priority version of the question to be the most compelling for various reasons. First, we believe it is less prone to social desirability bias. Since all of the values are potentially viewed as socially desirable, asking about each one individually could lead participants to simply state that all are important. Asking them to choose between them, however, forces them to identify those that are of utmost priority, even if all could be seen as desirable. Second, this type of question is likely more comparable across individuals. Likert-scales can be interpreted differently by different people, as the dividing line between agreeing "completely" and "somewhat" is less objectively obvious than what it means to prefer one thing to another. Finally, this type of measure is theoretically supported by much of the literature on the link between values and environmental behavior. Steg (2016) argues that the link between environmental values and related behaviors is

mitigated when individuals are operating in a choice environment where competing values are also at play. According to Schwartz, "attitudes and behavior are guided… by tradeoffs among competing values that are implicated simultaneously" (1996, p.121). Values may be culturally shared, but individuals prioritize those values differently, leading to different individual choices and actions in practice (Steg et al. 2014b). Thus, it is an individual's relative prioritization of values, not their absolute magnitude (which is difficult to measure in a comparable way across individuals anyway) that is the relevant driver of environmental behaviors.

This approach is similar to the strategy employed by Agrawal, Chhatre & Gerber (2015) in which they ask respondents what reason is more important for conserving forests and force them to choose between economic and environmental reasons.[8] Our measurement strategy differs in that it focuses specifically on values and includes a wider variety of values, based loosely on categorizations provided by the previous literature on values. An implication of this measurement strategy is that identification with one value is mechanically linked to the measure of others. Thus, an increase in the prioritization of environmental values must, by necessity, correspond with a decrease in the prioritization of other values. However, given the theoretical justification for a focus on relative prioritization of values, rather than absolute agreement with them, we view this as a design feature, rather than a bug, of our measurement strategy.

The survey also included five questions relating to environmental and social beliefs, asking respondents to what extent they agreed with various statements. These were designed to assess

[8] Agrawal, Chhatre & Gerber (2015) also mention in a footnote that they piloted a version of the question that allowed respondents to choose "both." When they did so, nearly all of the participants chose that option. This demonstrates the potential for social desirability bias in questions that do not require trade-offs between competing values.

two main perceptions that were deemed likely to change as a result of a PES-like program, based on the two major critiques in the literature identified earlier in this paper. The environmental beliefs questions aim to assess people's perceptions of a trade-off between environmental conservation (biospheric values) and economic growth (egocentric values). The social beliefs questions aimed to gauge participants' views on inequality and egalitarian norms.

4.4 Analytic Methods

We assess the effects of the intervention on self-stated environmental and social values and beliefs using a difference-in-differences analysis, comparing the change in relevant survey responses in the treatment group to the change in those same questions in the control group (for all those who answered the questions at both baseline and endline). This difference-in-differences approach is preferable, because it does not assume that the treatment and control group would have been identical absent the intervention, only that the trend would have been similar (Angrist & Pischke 2008). The parallel trends assumption is often violated when there is some sort of selection bias into the treatment group that is endogenous to the outcome variables (Besley & Case 2000). However, randomization into the treatment group solves the selection bias problem (Duflo & Kremer 2005). The combination of randomization with difference-in-differences is particularly robust, as the randomization means there are likely to be no systematic differences in unobservable characteristics of the sort that could violate the parallel trends assumption. The intervention was cluster-randomized at the village level, but balance tests suggest that the treatment and control group do not differ significantly with respect to most key variables at the household level (See Appendix A).

For the purpose of these analyses, the data were stacked, meaning they were structured as a panel dataset, with two observations for each household: one from the pre-treatment (baseline) survey and one from the post-treatment (endline) survey. For each model, we restrict the sample to only those households who answered each question at both baseline and endline. In the basic difference-in-differences model, each hypothesized effect of the intervention is regressed on a simple model including three explanatory variables: a dummy for whether the observation was in the treatment group or not, another indicating whether the observation was from the baseline or endline survey, and finally, an interaction term between the treatment and endline variables. The coefficient on this interaction term represents the effect of the intervention (Angrist & Pischke 2008; Puhani 2012).

4.4.1 Intent-to-Treat Approach

To explore the effectiveness of the intervention as delivered to the whole population (ie comparing those in the control communities to all those who were randomized into the treatment group regardless of whether they entered into an agreement), we first ran basic intent-to-treat models. In the equation below, we describe the basic linear version of our difference-in-differences models, using an intent-to-treat approach. Y_{ivt} is the outcome variable for person *i* in village *v* at time *t*. D_v represents the treatment, T_t represents the post-treatment period, and the interaction term, $D_v \cdot T_t$ takes on the value of 1 only for observations from the treatment group that were surveyed in the post-treatment period. The coefficient δ is the estimator for our treatment effect: the difference between the difference in the treatment group after the intervention and the difference in the control group over the same time period. As treatment, endline and the interaction are included in the model, the de facto reference category is individuals in the control group at baseline.

$$Y_{ivt} = \alpha + \beta D_v + \gamma T_t + \delta(D_v \cdot T_t) + \varepsilon_{ivt}$$

While the equation above represents the basic linear model specification, our final models take the form of either ordered logit (in the case of the categorical outcome variables) or logit (in the case of the binary outcome variables) models, all with clustered standard errors by village. Results from the basic intent-to-treat models can be found in Appendix C.

This basic intent-to-treat analysis applies the difference-in-differences analysis to a comparison of the time trend between all observations in the control group and all observations in the treatment group, including those who did not choose to sign up for agreements through the program. This is an appropriate approach because we wish to understand the overall impact of the program (including the effect of the offer itself and its likelihood to be adopted). It is also appropriate due to the likelihood that there are spillover effects within the treatment group, whereby those who did not directly participate as an agreement-holder might still be affected, for example through the spread of social norms from those in their village who did participate or through the effect of the information sessions delivered in all treatment villages.

In the case of the outcomes derived from the respondent's prioritization of environmental and social values, we also include one additional covariate to capture some heterogeneity in the number of responses offered. While the question asked the respondent to choose only the top two priorities, in some cases enumerators allowed respondents to name three. Thus, we include a control variable for the number of responses given, as this of course directly affects the likelihood of choosing any particular option from the list. Unsurprisingly, this variable is highly significant as a predictor of

choosing any particular response. Our main results are consistent whether this additional variable is included or not. No other covariates are included in the main model described above.

4.4.2 Intent-to-Treat Plus Matching

The randomized nature of the program implementation should eliminate the need to include control variables (Mutz 2011). However, because the subsample that received the values questions was not randomly selected and did differ slightly from the broader sample, this introduces the possibility that the members of the treatment and control groups who were included in the final sample differ in some systematic way. Though we have no reason to suspect that there is selection bias into this smaller sample that is systematically related to our outcomes of interest, we cannot wholly rule it out. Thus, to address this possibility, we also include a matching analysis to further confirm the validity of our results. While matching alone cannot guarantee causal inference, it may be combined with traditional ITT estimates to strengthen confidence in the results (Sekhon 2009). Using Stata's psmatch2 command and the mahalanobis distance matching specification with replacement (Leuven & Sianesi 2018), we identify matched pairs between the treatment and control group. The mahalanobis distance matching uses a specified set of covariates from the baseline survey, based on prior research into the key determinants of taking up the agreements (Grillos 2017).

The covariates used in the matching include demographic controls (the age and education of the head of household) as well as a combination of financial factors (formal land ownership, cattle ownership, the number of rooms in the home, whether the household has alternative sources of income apart from that derived from the land, whether anyone in the household has taken out loans

448 in recent months, trust in institutions), community involvement (participation in community work
449 projects, generations living in the community, participation in the formal community decision-
450 making body), and environmental values (inability to identify forest benefits, whether they
451 prioritized environment as a value at baseline, and agreement with various statements about
452 environmental conservation). These covariates were used to identify a matched sample based on
453 baseline characteristics, which were then incorporated into the basic intent-to-treat regression
454 models using frequency weights. (Full output of the regression models based on the matched
455 sample can be found in Appendix E.)
456
457 *4.4.3 Instrumental Variables Approach (Take-up)*
458 The intent to treat model considers outcomes of all households in the treatment community the
459 same, regardless of whether they took up the treatment or not. However, if we believe that the
460 intervention should only have an effect on those who directly entered into *Watershared*
461 agreements, then it is appropriate to instead calculate the Complier Average Causal Effect
462 (CACE). In our case of two-sided noncompliance, one widely accepted method of estimating the
463 effect of the treatment on the treated is to use treatment assignment as an instrumental variable
464 predicting take-up (Gerber & Green 2012; Glennerster & Takavarasha 2013). Because treatment
465 assignment was randomly assigned and affects outcomes through its effect on actual treatment, it
466 is an ideal instrumental variable. It can be used to estimate the treatment effect through a two-stage
467 least squares regression process in which we estimate predicted take-up as a function of treatment
468 assignment, and then use that predicted take-up as the key predictor of our outcomes of interest.
469

However, this approach assumes that the instrumental variable (treatment assignment) can only affect outcomes through take-up of the *Watershared* agreements (Gerber & Green 2012; Glennerster & Takavarasha 2013). Thus, this approach is likely inappropriate in the case of this intervention, where treatment assignment means exposure to framed information sessions with potential effects on those in the treatment group, even if they do not ultimately take up agreements. We include the instrumental variables approach mainly as a robustness check related to the issue of two-sided noncompliance. We apply it using Stata's ivregress command, specifying treatment assignment as the instrument for agreement take-up. The instrumental variables regressions can be found in Appendix D, and their results are consistent with those of our basic intent-to-treat models.

4.4.4 Predicted Probabilities and Other Robustness Checks

Some scholars argue that, in logit models, interaction terms should not be interpreted the same way as in other models, and that a statistically significant interaction term is neither necessary nor sufficient for a true interaction to exist (Ai & Norton 2003; Berry et al. 2010). Instead they recommend focusing on predicted probabilities. To address this potential critique, for our two binary outcome variables (relating to the prioritization of environmental and social values, respectively), we also confirm our main results using predicted probabilities (Berry et al. 2010). See Appendix F for the test of second differences confirming our finding with respect to environmental values. Though not presented in the paper, we have also confirmed that results are robust to the use of 2 or 3 nearest neighbors, as opposed to 1, to the application of the instrumental variables regression to the matched dataset, and to a matched comparison of endline values only as opposed to the difference-in-differences estimator.

5. Results

Using pre- and post-intervention data from a randomized controlled trial, we measured the causal effect of *Watershared* on environmental and social values and beliefs. In Table 1 below, we summarize the findings to come out of three different models: (1) an approach that uses treatment assignment as an instrumental variable to measure the effect of entering into agreements (i.e. take-up), (2) an intent-to-treat approach that measures the effect of being in a village where agreements were offered, irrespective of individual take-up, and (3) the intent-to-treat model applied to a matched sample, where the treatment and control group has been selected to be as similar as possible, based on baseline characteristics previously shown to influence take-up of agreements. The first row in Table 2 shows the mean value of each variable at baseline (for the full sample used in analysis), and the subsequent rows show the treatment effect on that outcome variable that is attributable to the intervention, as predicted by each analytical approach. (Full output from the regression models used to generate this table can be found in Appendix C-E.)

Table 2: Synthesis of the model results

	Environ. Values	Environmental Beliefs		Social Values	Social Beliefs		
	Prioritizes environment as value for kids	"Must harm environment to improve life"	"Environ-ment improves incomes"	Prioritizes sharing/ altruism as value for kids	"Earn more, should share with others"	"Work more, should earn more"	"Govern ment responsib le address inequality"
Baseline Mean	0.414	1.432	4.621	0.237	2.886	4.636	3.320
Take-Up	0.542*	-0.143	-0.020	-0.440*	0.187	1.806***	1.491*
Intent to Treat	0.199**	-0.058	-0.004	-0.142*	0.258	0.513***	0.485*
(+ Matching)	0.285**	-0.075	-0.047	-0.064	0.085	0.632**	0.525+

No significant effect
Positively influenced
Negatively influenced

$^{+}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$

Our results show that the intervention increases the likelihood that people choose environmental protection as a value that should be prioritized for their children, suggesting that *Watershared* may

510 have resulted in 'crowding in' of environmental values. In addition, the intervention alters social
511 beliefs within the treated communities. It tends to support an individualistic and/or meritocratic
512 view as opposed to a more communalized model of local redistribution. At the same time, it also
513 increased the view that the government should play a direct role in addressing inequality. Below,
514 we discuss results with respect to each of our key outcome variables in more detail.
515
516 **5.1 Environmental Values & Beliefs: Crowding in of Environmental Values**
517 Prior to the intervention, fewer than half (~41%) of respondents prioritized "protecting the
518 environment" as one of the most important values to teach their children. After the intervention,
519 people in the treatment group were more likely to prioritize environmental protection (Figure 1).
520 This result was both highly significant across all three model types and relatively large in
521 magnitude. The difference in differences is estimated to be 0.285, meaning that the estimated effect
522 of the intervention was for an additional ~28% of participants to prioritize environmental values
523 who previously did not. This demonstrates a "crowding in" of environmental values.
524
525 Figure 1 illustrates the shift in prioritization of environmental values. On the left-hand side, we
526 show (for the matched sample) the raw proportion of participants who rank environmental values
527 above others for (i) the control group, (ii) those in the treatment group who did not take up
528 agreements, and (iii) those in the treatment group who did take up agreements, both before and
529 after the intervention. This shows that the proportion prioritizing the environment increased in the
530 treatment group both for those with and without agreements, although the jump is larger for the
531 agreement-holders. On the right-hand side of Figure 1, we show the predicted proportions based
532 on the intent-to-treat model plus matching. It demonstrates that, on the whole, the intervention

group increased their prioritization of environmental values to a statistically significant degree, while the control group remained more or less constant.

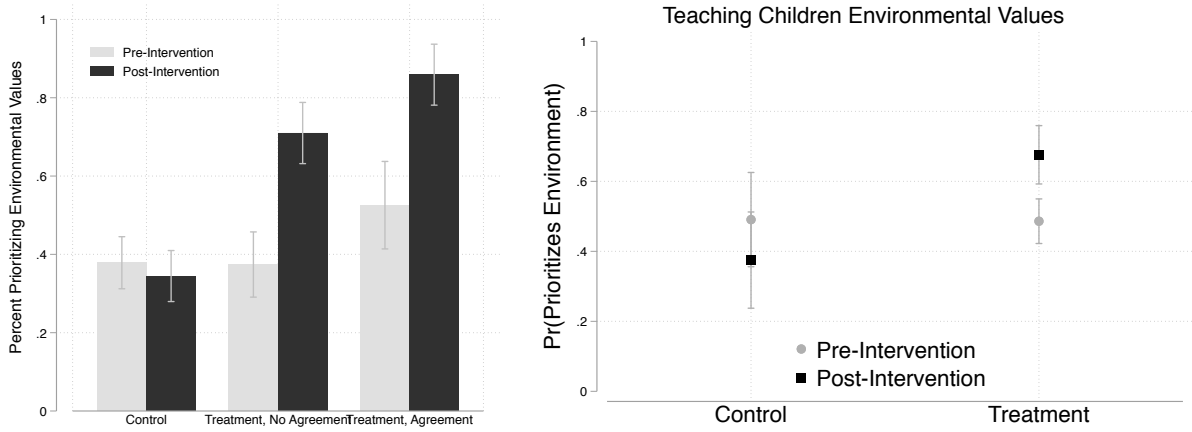

Figure 1: Prioritization of Environmental Values
(Left: Distribution of Raw Data –Matched Sample, Right: Predicted Probabilities from Matched Regression Model)

Descriptive statistics and visual inspection of the raw data suggest that the increase in prioritization of environmental values occurred among both agreement-holders and non-agreement-holders in the treatment group. Community members within the treatment villages who did not sign compensation agreements would still have received the informational components of the intervention, which the NGO delivered through community meetings as part of the initial program offer. Thus, residents of the treatment villages would have been exposed to the reciprocity framing even if they did not receive any compensation, and be subject to any socialization effects that could arise from the communication alone.

With respect to environmental beliefs, there was no statistically significant effect on how likely a person was to agree with the statements “To improve quality of life, it is necessary to harm the environment” and “We can improve our incomes if we protect the environment”. Most people in both groups already strongly agreed with pro-environment statements at baseline (more than 75%

choosing the most extreme pro-environmental position on a 5-point likert scale on each question) and these opinions remained relatively stable over the period of the study.

5.2 Social Values & Beliefs: Altered Beliefs on Inequality and Government Involvement

In the treatment group, there was a negative shift in the likelihood that a household prioritizes sharing or altruism as a value to teach their children. This effect was statistically significant in two of our three models, but it was not robust to the use of the matched sample. This implies that the shift likely was related to particular characteristics of those sampled in the treatment group, rather than a result of the intervention itself. We thus hesitate to put too much weight on this finding, but it would be consistent with other results discussed below, including a regional trend toward individualism, and the notion that the intervention may have strengthened or accelerated that existing trend.

With respect to the first of the three questions on social beliefs, the intervention had no effect on agreement with the statement “If a person earns more than others, they must share with the rest”, but more than 45% of people at baseline already disagreed or strongly disagreed with that statement. That percentage increased to more than 70% in the treatment group after the intervention but as disagreement increased in the control group as well, this effect was not statistically significant and cannot be directly attributed to the intervention. In both the treatment and control group, individuals are less likely at endline to favor the form of redistribution suggested by this question. This may be interpreted as a general trend toward individualism across the region over time (in both treatment and control groups), unrelated to the *Watershared* intervention.

Relatedly, after exposure to the intervention, treated respondents were more likely to agree that "If a person works more than others, it is fair that they earn more money" (Figure 2). This result was statistically significant and consistent across all model specifications. While most people agreed with this statement even at baseline, an increase in the percentage of people in the treatment group stating that they "strongly agree" (the most extreme option on a 5-point Likert scale) is what drives the change in the treatment group. This reflects an increased identification with the notion of "meritocratic inequality." It suggests that the intervention may have further strengthened existing trends toward individualism in the region, as evidenced by the result described in the previous paragraph.

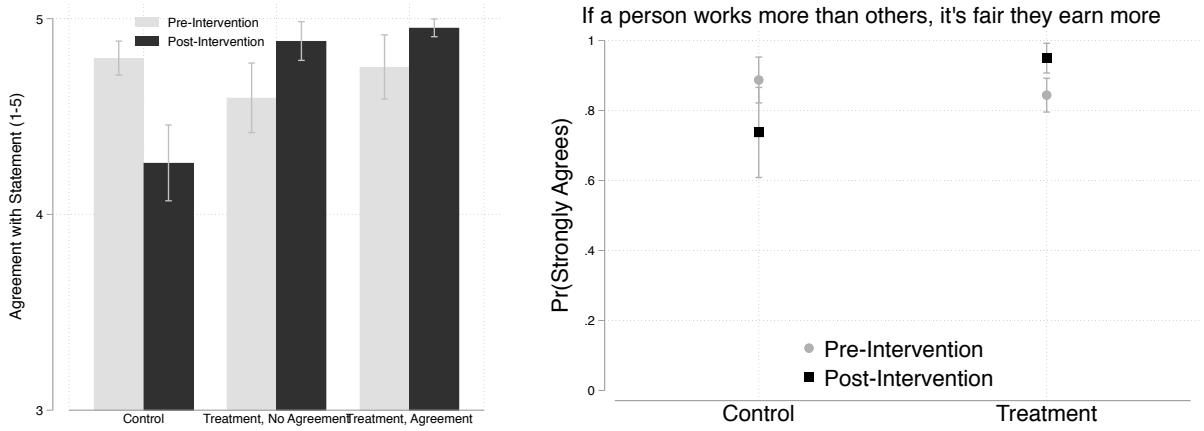

Figure 2: Agreement with Meritocratic Inequality
(Left: Distribution of Raw Data – Matched Sample, Right: Predicted Probabilities from Matched Regression Model)

However, there was also a positive effect on agreement with the statement "It's the responsibility of the government to reduce inequality of income between people with a lot of money and people with a little money" (Figure 3). In the matching analysis with one nearest neighbor, the statistical significance for this latter outcome drops but is still marginally significant (p=0.069). (Using 2 or 3 nearest neighbors, the p value is below 0.05.) This result with respect to government responsibility moves in the opposite direction of the regional trend. (In the control group, support

for government involvement decreases over this same time period.) We view this result as demonstrating an increased belief that the social security net should be transferred from the community to the government level. After experiencing this intervention (presented as a government-NGO partnership), individuals in the treatment communities are more likely to believe that the government should shoulder the responsibility for helping out the poor.

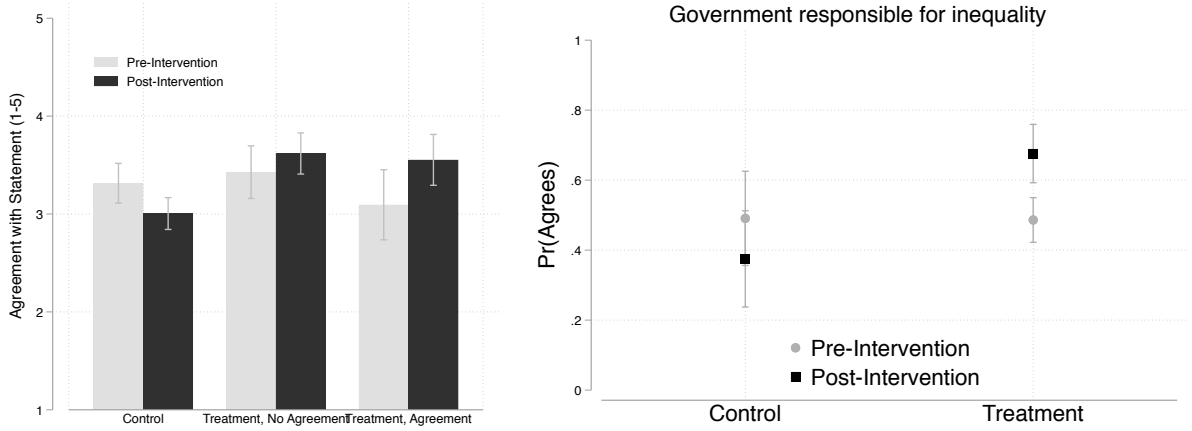

Figure 3 Agreement with Government Responsible for Inequality
(Left: Distribution of Raw Data – Matched Sample, Right: Predicted Probabilities from Matched Regression Model)

Respondents simultaneously felt that income inequalities cannot be the responsibility of local villagers themselves, but that they must be dealt with somehow through government intervention. Taken as a whole, we interpret these results as an indication that the treatment increased the acceptability of government intervention with respect to income inequality, despite a strong regional trend toward disagreement with redistribution in general. The treatment provoked a change in local perception of distributional values, from a community-based system to a state-based system. After exposure to the intervention (presented as a partnership with municipal governments), respondents are more likely than the control group to agree with meritocratic inequality but also to support the role of government in addressing inequalities. In general, the

612 program had a countervailing effect on the overall regional trend toward individualism and limited
613 government involvement, by increasing the acceptability of government intervention.
614
615 **6. Discussion**
616 Understanding the effects of incentive programs like PES on environmental and social values is
617 important in order to improve the chances for such interventions to make deep and permanent
618 socio-ecological change toward more sustainable development. Two major concerns in the
619 literature have been (i) the risk of crowding out values that are supportive of conservation behavior
620 (Rode, Gómez-Baggethun & Krause 2015, Ezzine-de-Blas et al. 2019) and (ii) the tension between
621 market-based efficiency and the equity of PES interventions (Pascual et al. 2010). Our study
622 speaks to both of these ongoing discussions.
623
624 First, our case illustrates that, contrary to fears around motivation crowding, *Watershared* had a
625 positive influence on self-stated pro-environmental values. Notably, this program influenced
626 environmental values even among those who did not receive any compensation. This suggests not
627 only that the program avoided the crowding out often associated with financial incentives, but that
628 it did so, at least in part, through the introductory information sessions, which included framing
629 related to reciprocity. This complements other literature which emphasizes the role of 'nudges', or
630 subtle contextual cues – as opposed to direct information about outcomes – in influencing
631 environmental behaviors (Thaler & Sunstein 2008, Ölander & Thogersen 2014). Since framed
632 information sessions are generally inexpensive relative to other programmatic design features, this
633 also represents a promising and cost-effective approach for policy-makers.
634

635 Our results also illustrate that the scheme had influences on social beliefs. The program was
636 associated with an increased acceptance of 'meritocratic inequality' but also increased support for
637 government involvement in reducing inequality. Prior work demonstrated that barriers to entry
638 (such as a lack of formal land title) limit the ability of the poorest community members to
639 participate in this program (Grillos 2017), echoing concerns in the PES literature that barriers to
640 entry could potentially exacerbate pre-existing inequalities. But our results here show that even
641 among those in the treatment group who have not entered into *Watershared* agreements, the shift
642 in social beliefs moves in the same direction. That is to say, people who have *not* directly benefitted
643 from the compensations (but have been exposed to the reciprocity framing) *also* strongly agree
644 with the meritocratic inequality statement and simultaneously favor government action on
645 inequality. This echoes another recent study which concluded that payments programs based on
646 meritocratic principles need not be in conflict with equity (Loft et al. 2019).
647
648 Our results with respect to views on the role of government could, at first glance, be interpreted as
649 increased support for redistribution, but our other results contradict the notion that there is support
650 for direct redistribution between people within the community. (Most people in both the treatment
651 and control group disagree that individuals are obligated to share their wealth.) Instead this result
652 seems to speak directly to views about the government itself. This intervention was conducted as
653 a collaborative effort between Natura and several municipal governments. (Though the municipal
654 governments did not directly contribute funds in the early years of the intervention during which
655 these data were collected, the program was always presented to the communities as being
656 conducted in partnership with the municipal government.) As a result of this quasi-governmental
657 intervention, people's views of government and its role have been altered. Contrary to concerns in

658 the literature that disenchantment with program-related inequalities may decrease support for later
659 efforts, in this case we find that there may in fact be greater support for future governmental policy
660 interventions as a result of this particular program.
661
662 The use of a Randomized Controlled Trial to evaluate impacts of this program lends increased
663 internal validity to our study, providing one of the clearest examples of causal inference in this
664 literature to date. However, there are of course still limitations associated with our research design
665 that we wish to acknowledge here. First, the use of self-stated data on values and beliefs is limited
666 by social desirability bias and experimenter demand effects, in that respondents may say what they
667 believe researchers want to hear (Tourangeau et al. 2000). This is somewhat mitigated by the fact
668 that we collect pre- and post- intervention data in both treatment and control groups (since social
669 desirability bias is likely to be at play across all interviews), and the research team made efforts to
670 ensure that the interviewers were not seen as affiliated with the NGO (while Natura did manage
671 the initial hiring of the enumerators, the unaffiliated researchers trained and supervised them).
672 Second, given that this experiment spanned multiple years, we cannot completely rule out the
673 possibility of spillover effects. If control communities heard about incentive programs in other
674 villages, they may perceive that others have opportunities to earn more to which they have not had
675 access – thus decreasing their degree of comfort with the notion of meritocratic inequality. This is
676 of particular concern for the meritocratic inequality finding, since a corresponding *decrease* in
677 agreement in the control group contributes to the statistical significance of the result.
678
679 Our results contrast with other prior studies that found evidence of motivation crowding in
680 conservation incentive programs (Agrawal, Chhatre & Gerber 2015; Chervier, Le Velly & Ezzine-

681 de-Blas 2019; García-Amado, Pérez & García 2013; Moros, Valez & Corbera 2019), and
682 corroborate arguments that fears of motivation crowding in PES may be overstated (Andersson et
683 al. 2018; Handberg & Angelsen 2019; Kaczan, Swallow & Adamowicz 2019). However, we are
684 cautious in generalizing these results to other incentive programs, as we believe contrasting results
685 are due to differences in specific design features. In particular, we believe the use of in-kind
686 compensation, individual-level agreements, and the targeted framing related to local reciprocity
687 norms all likely influenced the results we present here.
688
689 As described earlier, there is reason to believe that both the use of in-kind payments and norm-
690 based framing may reduce the risk of crowding out. Here we demonstrate that these design features
691 may go even farther, leading to a *crowding in* of environmental values. At the same time, the
692 reciprocity framing may also have influenced results with respect to social beliefs. One
693 interpretation of reciprocity is an expectation of fair exchange, including potentially that of reward
694 for effort.[9] The Natura information sessions evoked reciprocity not only with respect to human-
695 environment relations generally but also specifically with respect to the compensations earned
696 through the conservation agreements. If framing successfully engaged internalized reciprocity
697 norms with respect to environmental protection, then perhaps it also led individuals to feel more
698 entitled to the goods earned through the agreements – and therefore more comfortable with any
699 potentially unequal distribution resulting from it.
700

[9] This interpretation is distinct from alternative definitions of reciprocity in, for example, the anthropological literature, which views reciprocity not as a direct exchange of goods and services, but rather as an indirect and delayed system of exchange based on trust and internal cohesion.

Finally, *Watershared* involves individual land owners entering into agreements. Several studies have suggested that individual payments have different psychological implications for motivation crowding compared with communal payments (Agrawal, Chhatre & Gerber 2015; Kerr, Vardhan and Jindal 2014; Midler et al. 2015; Moros, Vélez and Corbera 2019; Narloch, Pascual and Drucker 2012). A recent paper found that communal payments in Mexico had a positive impact on social capital (Alix-Garcia et al. 2018). Programs directed at entire communities may eliminate barriers faced by non-landowners, but on the other hand, community-based management programs are in practice often co-opted by local elites, potentially also resulting in elite capture of benefits (Bardhan & Mookherjee 2000; Iversen et al. 2006). Even if payments are not skewed within communities, they may influence inequality across groups, with benefits accruing disproportionately to wealthier communities relative to the additionality of their conservation (Murtinho & Wolff 2015). On the other hand, individual agreements can increase the perception of individual rights to natural resources and, as observed here, influence local attitudes toward more individualism. We thus recommend caution in assuming that the same results may be found in communal payment settings.

5.1 Conclusions

Our results are supportive of the continued use of incentives to promote conservation, and they highlight a particular approach that has successfully increased pro-environmental values (these show 'crowding-in' as opposed to 'crowding out'). We also provide relatively robust evidence about how a carefully designed incentive scheme may influence the perspectives of community members themselves, potentially affecting the acceptance and effectiveness of future policy efforts. This work contributes to a large body of social science research about how the form in

724 which incentives and information are delivered can shape human perceptions and values and
725 thereby subsequent policy intervention. It also engages with a growing, policy-relevant literature
726 on psychology and environmental behavior.
727

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1006 # Appendix

1007 **A. Balance Tests - Treatment vs. Control (based on full baseline survey)**

1008

	Control		Treated		
	Mean	SD	Mean	SD	p-value
Environmental Values & Beliefs:					
"Must harm environment to improve life" (1-5)	1.41	1.02	1.45	1.07	0.355
"Environment improves incomes" (1-5)	4.63	.85	4.60	.90	0.449
Prioritizes environment as value for kids	.38	.49	.40	.49	0.346
Social Values & Beliefs:					
"Earn more, must share with others" (1-5)	2.83	1.72	2.81	1.71	0.716
"Work more, should earn more" (1-5)	4.69	.89	4.63	.98	0.119
"Government responsible address inequality" (1-5)	3.44	1.51	3.43	1.54	0.810
Prioritizes sharing and altruism as value	.25	.43	.22	.42	0.084^{+}
Demographic Controls:					
Age Head of Household	49.87	16.37	49.36	16.37	0.429
Educational Level	4.65	3.47	4.82	3.67	0.225
People in Household	3.50	1.88	3.50	1.81	0.960
OTB Membership	.84	.37	.76	.42	0.000***
Hectares of Land Owned	25.43	61.77	26.58	63.04	0.644
Cattle Ownership	.69	.46	.69	.46	0.753
Number of Cattle	12.09	22.60	11.41	17.65	0.384
Perceptions of Current Situation:					
Forest better than 5 years ago	2.19	.81	2.22	.82	0.350
Community care better than 5 years ago	1.97	.77	2.03	.77	0.026*

Both water quality & quantity is a problem	.20	.40	.23	.42	0.137
All victims of problem	.87	.33	.90	.30	0.051^{+}
All contribute to solution	.75	.43	.78	.42	0.205
Total Observations (exact n varies by variable)	1,158		1,443		

$^{+}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$

B. Balance Tests – Included vs Excluded from Analyses (based on baseline survey)

	All Baseline		Re-surveyed only			Values Questions Subset		
Environmental Values & Beliefs:	Mean	sd	Mean	sd	p-value	Mean	sd	p-value
"Must harm environment to improve life" (1-5)	1.43	1.05	1.43	1.06	0.797	1.48	1.12	0.414
"Environment improves incomes" (1-5)	4.62	.88	4.62	.88	0.651	4.63	.87	0.770
Prioritizes environment as value for kids.	.39	.49	.40	.49	0.126	.41	.49	0.427
Social Values & Beliefs:								
"Earn more, must share with others" (1-5)	2.82	1.72	2.80	1.70	0.472	2.89	1.73	0.435
"Work more, should earn more" (1-5)	4.66	.94	4.64	.98	0.266	4.64	.97	0.675
"Government responsible address inequality"(1-5)	3.43	1.52	3.44	1.53	0.654	3.32	1.57	0.148
Prioritizes sharing and altruism as value	.24	.42	.24	.42	0.991	.23	.42	0.796
Demographic Controls:								
Age Head of Household	49.59	16.37	49.62	15.44	0.909	49.27	16.69	0.700
Educational Level	4.74	3.58	4.85	3.54	0.053^{+}	4.84	3.58	0.589
People in Household	3.50	1.84	3.66	1.78	0.000***	3.43	1.76	0.457
OTB Membership	.80	.40	.82	.39	0.001**	.78	.41	0.411
Hectares of Land Owned	26.07	62.46	26.85	64.10	0.399	32.36	84.57	0.051^{+}
Cattle Ownership	.69	.46	.75	.43	0.000***	.69	.46	0.952
Number of Cattle	11.71	20.00	13.10	21.42	0.000***	13.68	24.97	0.055
Perceptions of Current Situation:								
Forest better than 5 years ago	2.20	.81	2.22	.81	0.148	2.19	.83	0.805
Community care better than 5 years ago	2.00	.77	1.99	.77	0.232	1.97	.77	0.433
Both Water quality & quantity is a problem	.22	.41	.22	.41	0.751	.20	.40	0.367
All victims of problem	.89	.32	.89	.31	0.863	.86	.35	0.150
All contribute to solution	.76	.42	.76	.42	0.915	.76	.42	0.969
Treatment:								
Treatment Group	.55	.50	.58	.49	0.000***	.69	.46	0.000***
Agreement-Holder (Treatment Group only)	.38	.49	.47	.50	0.000***	.40	.49	0.528
Total Observations (exact n varies by variable)	2,601		1,672			333		

(p-values compare each subsample to the rest of the households picked up in the baseline survey)

$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

C. Basic Intent to Treat Regressions: Environmental & Social Values & Beliefs

	Environmental Beliefs & Values			Social Beliefs & Values			
	(1) "Must harm environment to improve life"	(2) "Environment improves incomes"	(3) Prioritizes environment as value for kids	(4) "Earn more, should share with others"	(5) "Work more, should earn more"	(6) "Government responsible address inequality"	(7) Prioritizes sharing/ altruism as value for kids
TreatmentEndline	-0.157	-0.0281	0.881**	0.326	1.971***	0.608*	-0.724*
	(0.182)	(0.189)	(0.328)	(0.338)	(0.564)	(0.304)	(0.362)
Treatment	0.00526	0.196	0.391	-0.0223	-0.106	-0.00324	-0.109
	(0.151)	(0.143)	(0.257)	(0.279)	(0.330)	(0.239)	(0.286)
Endline	0.279*	-0.287+	0.00453	-0.863***	-0.560+	-0.416*	0.378
	(0.138)	(0.158)	(0.259)	(0.262)	(0.291)	(0.199)	(0.273)
NumResponses			0.919***				1.144***
			(0.187)				(0.227)
Constant			-2.450***				-3.379***
			(0.435)				(0.518)
c1	1.458***	-3.352***		-0.757**	-2.996***	-1.662***	
	(0.117)	(0.147)		(0.238)	(0.268)	(0.202)	
c2	2.300***	-2.884***		0.0185	-2.585***	-0.902***	
	(0.135)	(0.143)		(0.240)	(0.302)	(0.183)	
c3	2.791***	-2.354***		0.162	-2.344***	-0.174	
	(0.151)	(0.122)		(0.245)	(0.285)	(0.162)	
c4	3.527***	-1.177***		1.214***	-1.703***	0.968***	
	(0.191)	(0.116)		(0.250)	(0.279)	(0.169)	
Observations	3290	3290	642	666	664	662	642

Standard errors in parentheses

$^{+}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$

D. Instrumental Variable Regression: Environmental & Social Values & Beliefs

	Environmental Beliefs & Values			Social Beliefs & Values			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	"Must harm environment to improve life"	"Environment improves incomes"	Prioritizes environment as value for kids	"Earn more, should share with others"	"Work more, should earn more"	"Government responsible address inequality"	Prioritizes sharing/ altruism as value for kids
TakeUp	-0.143	-0.0195	0.543*	0.187	1.806***	1.491*	-0.440*
	(0.181)	(0.188)	(0.231)	(0.692)	(0.454)	(0.608)	(0.196)
Treatment	0.0134	0.0535	0.0933	-0.0514	-0.0827	-0.0570	-0.0194
	(0.0646)	(0.0481)	(0.0600)	(0.216)	(0.109)	(0.173)	(0.0522)
Endline	-0.0195	-0.0879	0.00419	-0.719***	-0.412***	-0.330+	0.105*
	(0.0669)	(0.0726)	(0.0620)	(0.205)	(0.125)	(0.178)	(0.0534)
NumResponses			0.184***				0.229***
			(0.0332)				(0.0407)
Constant	1.424***	4.590***	-0.0161	2.922***	4.693***	3.360***	-0.205*
	(0.0531)	(0.0417)	(0.0840)	(0.185)	(0.0890)	(0.128)	(0.0927)
Observations	3290	3290	642	666	664	662	642

Standard errors in parentheses

$^{+}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$

Note: The use of the matched sample combined with the instrumental variable regression produces results consistent with this table, except that the coefficient on "prioritizes sharing" is no longer significant.

E. Matching Analysis (Nearest Neighbors=1): Environmental & Social Values & Beliefs

	Environmental Beliefs & Values			Social Beliefs & Values			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	"Must harm environment to improve life"	"Environment improves incomes"	Prioritizes environment as value for kids	"Earn more, should share with others"	"Work more, should earn more"	"Government responsible address inequality"	Prioritizes sharing/ altruism as value for kids
TreatmentEndline	-0.239	-0.144	1.246**	0.136	2.280***	0.672+	-0.330
	(0.222)	(0.226)	(0.392)	(0.421)	(0.682)	(0.369)	(0.427)
Treatment	0.188	0.177	0.182	0.188	-0.375	0.0299	-0.0512
	(0.165)	(0.174)	(0.313)	(0.301)	(0.378)	(0.343)	(0.357)
Endline	0.377*	-0.233	-0.338	-0.632+	-1.032*	-0.431	0.122
	(0.186)	(0.196)	(0.361)	(0.370)	(0.486)	(0.304)	(0.401)
NumResponses			1.002***				0.927**
			(0.256)				(0.282)
Constant			-2.462***				-2.984***
			(0.554)				(0.620)
c1	1.720***	-3.341***		-0.463+	-3.264***	-1.699***	
	(0.130)	(0.194)		(0.276)	(0.305)	(0.317)	
c2	2.552***	-2.951***		0.210	-2.955***	-0.910**	
	(0.153)	(0.174)		(0.285)	(0.331)	(0.313)	
c3	2.972***	-2.447***		0.376	-2.743***	-0.0724	
	(0.175)	(0.161)		(0.282)	(0.334)	(0.285)	
c4	3.701***	-1.285***		1.498***	-2.064***	1.031**	
	(0.216)	(0.152)		(0.278)	(0.334)	(0.318)	
Observations	3564	3560	836	864	864	864	836

Standard errors in parentheses

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: Using 2 or 3 nearest neighbors rather than 1 produces results consistent with this table (and increases statistical significance in model 6.)

F. Test of Second Differences: Prioritization of Environmental Values

	Pr(Prioritizes Environmental Values)	Test of First Difference	Test of Second Difference
Control Group			
Pre-Intervention	0.44 (0.06)	0.36 – 0.44 = -0.08 (p=0.342)	0.21 – -0.08 = 0.28** (p=0.001)
Post-Intervention	0.36 (0.07)		
Treatment Group			
Pre-Intervention	0.48 (0.04)	0.69 – 0.48 = 0.21*** (p=0.000)	
Post-Intervention	0.69 (0.04)		

Standard errors in parentheses

$^{+}\ p < 0.1$, $^{*}\ p < 0.05$, $^{**}\ p < 0.01$, $^{***}\ p < 0.001$

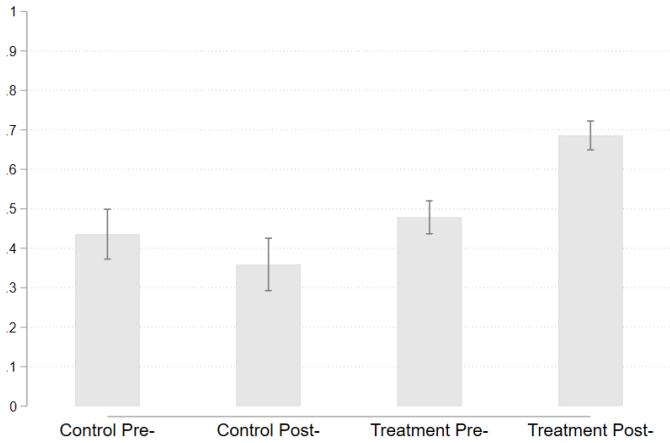

Figure 4: Predicted Probabilities, Prioritizing Environment